Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

THE PRINCIPLES OF MECHANICS WITH NEWTON

FROM 1666 TO 1679.

NEWTON'S achievements with respect to the principles of mechanics have been considered by Mach.[1] I have tried to supplement this account by a thorough study of what is known or can be concluded about the development of Newton's ideas on the principles of mechanics. For this purpose it was necessary, above all, to make use of the manuscripts of Newton prior to the publication (1687) of the *Principia.* Many of these manuscripts have been carefully preserved in the library of Cambridge University, and these I have examined, thereby bringing to light a fact of importance, showing the occurrence of the conception of mass with Newton at an early date. Of the other manuscripts of Newton, the most important of those in the possession of the Royal Society of London and Trinity College, Cambridge, and some others, have already been published, and the new views suggested thereby—such as that of the important place which the determination of the attraction of a sphere of gravitating matter or an external point holds in the series of Newton's discoveries—require exposition and discussion, especially as they are not referred to by Mach.

---

[1] *Mechanics,* pp. 187-245. Dühring (*Kritische Geschichte der allgemeinen Principien der Mechanik,* 3d ed., Leipsic, 1887, pp. 172-211) gave an account of Newton's achievements, but by not examining Newton's conception of mass he neglected the discussion of the most important of these achievements—at least from the point of view of the principles of mechanics.

Indeed, the more we learn of Newton's work prior to 1687 and of the work of Newton's contemporaries, the more do we seem to be forced to the opinion that the importance of Newton's work was mainly due to the great mathematical advance which he made by the invention of his "method of fluxions" and his application of it to the problems of dynamical astronomy. It is not, as I think I have shown,[2] by a feat of the imagination that Newton is distinguished from his contemporaries; and the conception of mass and the principle of reaction, though never so clearly stated before Newton, had been accepted and used by many before Newton. Newton, owing to his mathematical ability of precisely formulating the principles he used in his mathematical physics, gave these principles a fairly clear expression,—and an expression in which many people, even at the present day, are acquainted with them to the exclusion of all other views, all historical knowledge, and all criticism. This one-sidedness must be regarded as most unfortunate, especially when certain defects in the Newtonian formulations are so plain to modern eyes.

Historical investigations bring before us the efforts of Newton's predecessors and contemporaries, and give us a truer idea of what Newton accomplished. Also these investigations sometimes serve the purpose of establishing the independence of two discoverers. In the present case we shall see most clearly that Newton's first investigations on circular orbits could not have been influenced by the results which, no doubt, Huygens had already obtained.

The present paper deals with Newton's work on mechanics from 1666 to 1679.

I.

The attempt to represent the movements of the heavenly bodies by a simple mathematical process was, apparently, first made by Eudoxus of Cnidus[3] in Asia Minor (408-355 B. C.). According to him, the earth was the center of the universe, and round it rotated with uniform velocity spheres carrying the stars, sun, and so on. The irregularities in the motions of the sun and moon which were soon found to exist were explained by Hipparchus of Rhodes (about 190-120 B.C.) by supposing that, though the sun and moon moved uniformly in their orbits, yet the center of their orbits was not the center of the earth. The great successor of Hipparchus was Claudius Ptolemy of Alexandria, who carried on the work of astronomical observation from about 127 to 150 A. D. He worked out an extremely elaborate scheme by which he represented the motions of the planets with considerable accuracy by means of a complex apparatus of epicycles.

---

[2] *Monist*, July, 1913.

For more than 1300 years the Ptolemaic system remained without serious challenge, and it was Nicolas Copernicus[4] (1473-1543) who observed that the celestial motions could be described far more simply if the sun were supposed to be the fixed center of the planetary system and the earth and the other planets to revolve round it. This great discovery was only communicated orally by Copernicus to his disciples, on account of possible theological difficulties, but shortly before his death his great work *De revolutionibus orbium caelestium* was published by Rheticus (1543) with a dedication by Copernicus to the pope, and with the remark that it is not necessary that astronomical hypotheses be either true or probable, but that they accomplish their object if they reconcile calculation with observation. The earth having been abandoned as the center of the universe, a further sacrifice had to be made: the principle of uniform motion in a circle had also to be given up. This came as a result of the more refined observations of the Dane Tycho Brahe[5] (1546-1601).

---

[3] Cf. E. W. Maunder, *The Science of the Stars,* London and Edinburgh, 1912, pp. 20-31.

[4] Brewster, *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton,* Edinburgh, 1855, Vol. I, pp. 253-258.

The observations of Tycho Brahe enabled his friend and pupil Johann Kepler[6] (1571-1630) to subject the planetary motions to a far more searching examination than had yet been attempted. Kepler first endeavored to represent the planetary orbits by the hypothesis of a uniform motion in circular orbits; but, in examining the orbit of Mars, he found the deviations from a circle too great to be owing to errors of observation. He therefore compared the observations with various other curves, and was led to the discovery that Mars revolves round the sun in an elliptical orbit in one of the foci of which the sun is placed. By means of the same observations, he found that the radius vector drawn from the sun to Mars describes equal areas in equal times. These two discoveries were extended to all the other planets of the system and were published at Prague in 1609 in his *Nova astronomia seu physica caelestia tradita commentariis de motibus stellae Martis.* In 1619 he published at Linz his *Harmonia mundi,* containing his third great discovery that the periodic times of any two planets in the system are to one another as the cubes of their distances from the sun.

Kepler also made speculations on the subject of gravitation, and these and other theories of gravitation—for example, of Descartes, Bouillaud, Borelli, Huygens, and Hooke—before Newton, are described by Rosenberger in the book cited below.[7]

II.

The impressive nature of many of the Newtonian discoveries in mechanics tends to hide what is, perhaps, the most important aspect of Newton's work. Newton put into our hands the key which unlocks many of the secrets of astronomy, such as the motions and attractions of sun and planets, and most people—even among scientific people—allow their admiration of Newton's results in celestial mechanics completely to hide what ought to be their admiration of what Newton did for the principles of dynamics. As Dühring[8] says: "The achievements of Newton in our domain [the principles of mechanics] are too often only appreciated under the influence of the applications of mechanics to the planetary system, and thus, especially by the English, relatively overestimated. By this, not only has an injustice been done to Galileo and Huygens, but that popular kind of judgment has been adopted in which the measure of appreciation is gauged, not by the consideration of the advances in point of principle but by the completed fact of a notable application and the astonishment at gigantic dimensions and discoveries so surprising in their spontaneity (*Unmotivirtheit*)." Lagrange,[9] comparing Galileo's telescopic discoveries with his contributions to mechanics, says that his mechanical investigations "did not bring him, in his life-time, as much celebrity as those discoveries which he made about the system of the world, but they are to-day the most enduring and real part of the glory of this great man. The discoveries of Jupiter's satellites, of the phases of the moon, of the sun-spots, and so on, only needed telescopes and assiduity; but extraordinary genius was needed to disentangle the laws of nature from phenomena which are always going on under our eyes, but of which the explanation had always eluded the search of philosophers."[10] We are tempted to remark that, in Newton's case, only mathematical talent and the method of fluxions (or the differential and integral calculus) were needed to work out the paths of the planets under their mutual attractions; but extraordinary genius was needed to discover the fundamental principles of mechanics which were necessary and sufficient in order that the application of mathematics might completely solve the problems of mechanics. However, we must remember that it was Newton himself who discovered the method of fluxions; while, on the other hand, no reasonable person can seriously maintain that Newton's formulation of the principles was wholly clear and cannot be improved upon. It is more correct to say that he succeeded in giving what is, in essentials, a *sufficient* foundation for mechanics: the foundation is not wholly *necessary*, in view of the facts that there is much tautology and at least one entity—mass —is "defined" by an apparent definition which really involves a vicious circle.

---

[5] *Ibid.,* pp. 258-263.

[6] *Ibid.,* pp. 263-270.

[7] Pp. 135-157. Cf. Brewster, *op. cit.,* pp. 282-288.

---

[8] *Op. cit.*, p. 172.

[9] *Méchanique analitique*, Paris, 1788, p. 159; cf. *Œuvres de Lagrange*, Vol. XI, pp. 237-238.

[10] On Galileo's work, cf. also Brewster, *op. cit.*, pp. 270-282.

We are going to try to ascertain the growth of Newton's ideas on the principles of mechanics. This is made difficult by three circumstances: (1) In all historical or critical accounts known to me of Newton's contributions to the principles, only Newton's *Principia* of 1687, and not his earlier manuscripts and other sources of information, have been analyzed; (2) in those publications where some account is given of Newton's earlier manuscripts, attention has been paid, almost to the exclusion of everything else, to the growth of his methods for dealing with astronomical problems; (3) the interest of both the contemporaries of Newton and their successors has been fixed, for the reasons stated above, on Newton's discoveries in celestial mechanics, and consequently we find—as in all branches of science until, comparatively speaking, recent years—that fundamental principles, especially when the deductions from them impress us by their grandeur, are often accepted uncritically. If science is compared to a tree—which, we must remember, grows both upwards and downwards—we may say that the average man of science does not care to busy himself with the roots when such a fine prospect can be attained by climbing. The life of a tree is in its roots; and science, if its roots are not cultivated, becomes worse than dead science—it becomes living pedantry.

I have made a thorough study of all the books which appeared to me to cast any light on the development of Newton's ideas on the principles of mechanics before 1687, and also of those manuscripts which Newton wrote before 1687 on mechanics, and which have either been printed or are preserved in the library of the University of Cambridge.

III.

I will first of all give a list of authorities consulted.

Stephen Peter Rigaud: *Historical Essay on the First Publication of Sir Isaac Newton's Principia,* Oxford, 1838.

J. Edleston: *Correspondence of Sir Isaac Newton and Professor Cotes, including Letters of Other Eminent Men, now first published from the originals in the Library of Trinity College, Cambridge; together with an Appendix containing other unpublished Letters and Papers by Newton; with Notes, Synoptical View of the Philosopher's Life, and a Variety of Details illustrative of his History,* London and Cambridge, 1850.

Sir David Brewster: *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton,* 2 vols., Edinburgh, 1855. A second edition—apparently unaltered, even as to the mistakes—was published at Edinburgh in 1860.

W. W. Rouse Ball: *An Essay on Newton's "Principia,"* London and New York, 1893.

Ferdinand Rosenberger: *Isaac Newton und seine physikalischen Principien. Ein Hauptstück aus der Entwickelungsgeschichte der modernen Physik,* Leipsic, 1895.

In addition to these, there is the Portsmouth Collection of scientific papers belonging to Newton, which is now in the possession of the University of Cambridge, and has been examined and arranged in the University library. A descriptive catalogue of it was published at Cambridge in 1888 under the title: *A Catalogue of the Portmouth Collection of Books and Papers written by or belonging to Sir Isaac Newton, the Scientific Portion of which has been presented by the Earl of Portsmouth to the University of Cambridge.* This catalogue was drawn up by the syndicate appointed on November 6, 1872, and the Preface is signed by H. R. Luard, G. G. Stokes, J. C. Adams, and G. D. Liveing.

An indication of the nature of the extant manuscripts and correspondence of Newton on scientific subjects is given in the above-cited book by Rouse Ball.[11] Both Rigaud and Brewster printed part of Newton's correspondence, but Rouse Ball has printed the most complete sets of correspondence between Hooke and Newton and between Halley and Newton. For these letters, then, I shall refer to Rouse Ball's book alone.

Fifty-nine letters from and to Newton were published by Rigaud in the second volume of *Correspondence of Scientific Men of the Seventeenth Century, including Letters of Barrow, Flamsteed, Wallis, and Newton, printed from the Originals in the Collection of the Right Honourable the Earl of Macclesfield,*[12] and a table of contents and index were added by A. De Morgan[13] in 1862.[14] Neither name nor date appeared on the supplement itself.

---

[11] *Op. cit.*, pp. 2-5. Here I will remark that the above books will always, in what follows, be referred to by the names of their respective authors followed by "*op. cit.*" The reference "*Portsmouth Catalogue*" is to the catalogue just mentioned.

[12] Two volumes, Oxford, 1841. The book was posthumous and was edited by Rigaud's son, Stephen Jordan Rigaud. The letters in question are on pp. 281-437 of Vol. II, and begin with the date 1669. These letters do not seem to contain anything of interest in connection with our present subject, which has not passed over into Brewster. Pp. 242-254 are interesting in connection with the respective merits of Gregory's and Newton's telescopes. The first volume seems to contain little of interest.

We must also mention A. De Morgan's[15] review of Brewster's *Memoirs*.[16] De Morgan clearly showed that Brewster had succumbed to hero-worship, and had consequently been unjust to Leibniz and Flamsteed. It cannot be too strongly emphasized that De Morgan, by his deep historical researches, high standard of conduct, and eminent sanity of judgment, has made contributions of the utmost value to our knowledge and estimation of the character and religious opinions of Newton.

IV.

For the biography of Newton (1642-1727) the chief authorities are De Morgan, to whose essays we here refer once for all, Edleston, and Brewster. To Edleston's book is prefixed an admirable "Synoptical View of Newton's Life." We shall here be concerned almost wholly with his work up to and including the publication of the *Principia* in 1687. From a scientific point of view Newton's further life is of far less interest.

According to Brewster, the date of Newton's quitting Cambridge is written by his own hand in his commonplace book as 1665; and thus Brewster concluded that he left Cambridge "sometime before the 8th of August, 1665,[17] when the college was 'dismissed' on account of the plague, and it was therefore in the autumn of that year, and not in that of 1666 [as Pemberton said], that the apple is said to have fallen from the tree at Woolsthorpe, and suggested to Newton the idea of gravity." On the other hand, Newton himself said[18] that in the beginning of the year 1666 he procured a glass prism "to try therewith the phenomena of colors"; and, after further particulars, added:[19] "Amidst these thoughts I was forced from Cambridge by the intervening plague."

---

[13] According to p. 414 of the *Memoir of Augustus De Morgan*, by his wife Sophia Elizabeth De Morgan (London, 1882).

[14] Rouse Ball, *op. cit.*, p. 4. Here, too, is some information about the Macclesfield Collection.

[15] The review is unsigned, but. cf. (Mrs.) S. E. De Morgan's *Memoir of Augustus De Morgan*, p. 261.

[16] *The North British Review*, Vol. XXXIII, 1855, pp. 307-338.

[17] See also Brewster, *op. cit.*, Vol. I, note on p. 30.

After Newton's return to Cambridge on the disappearance of the plague, he was elected Minor Fellow on the first of October, 1667. He took his degree of Master of Arts in 1668. In 1669 Newton succeeded Isaac Barrow in the Lucasian professorship of mathematics.

With Rouse Ball,[20] we consider the origin and history of the *Principia* previous to its compilation and publication (1685-1687) under three heads, which deal

a. with Newton's investigations in 1666;

b. with his investigations in 1679;

c. with his investigations in 1684, of which the chief results are embodied in the *De motu* of 1685.

Newton alluded to the subject of his early investigations in a letter to Halley of June 20, 1686:

"That in one of my papers writ (I cannot say in what year, but I am sure some time before I had any correspondence with Mr. Oldenburg, and that's) above fifteen years ago, the proportion of the forces of the planets from the sun, reciprocally duplicate of their distances from him, is expressed, and the proportion of our gravity to the moon's *conatus recedendi a centro terrae* is calculated, though not accurately enough."[21]

On July 14, 1686, Newton wrote to Halley:[22]

". . . . but for the duplicate proportion I can affirm that I gathered it from Kepler's theorem about twenty years ago."

---

[18] *Ibid.*, p. 380.

[19] *Phil. Trans.*, Vol. VI, p. 3075.

[20] *Op. cit.*, p. 2.

[21] *Ibid.*, p. 157.

[22] *Ibid.*, p. 165.

In a memorandum written, perhaps, about 1714,[23] Newton wrote that he had said "in the Introduction to the book of Principles":

"I found the Method [of fluxions] by degrees in the years 1665 and 1666. In the beginning of the year 1665 I found the method of approximating Series and the Rule for reducing any dignity of any Binomial into such a series. The same year in May I found the method of tangents of Gregory and Slusius, and in November had the direct method of fluxions, and the next year in January had the theory of colors, and in May following I had entrance into the inverse method of fluxions. And the same year I began[24] to think of gravity extending to the orb of the moon, and having found out how to estimate the force with which [a] globe revolving within a sphere presses the surface of the sphere, from Kepler's rule of the periodical times of the planets being in a sesquialterate proportion of their distances from the centers of their orbs I deduced that the forces which keep the planets in their orbs must [be] reciprocally as the squares of their distances from the centers about which they revolve: and thereby compared the force requisite to keep the moon in her orb with the force of gravity at the surface of the earth, and found them answer pretty nearly. All this was in the two plague years of 1665 and 1666, for in those days I was in the prime of my age for invention, and minded mathematics and philosophy more than at any time since. What Mr. Huygens has published since about centrifugal forces I suppose he had before me. At length in the winter between the years 1676 and 1677 [probably this should be 1679 and 1680] I found the proposition that by a centrifugal force reciprocally as the square of the distance a planet must revolve in an ellipse about the center of the force placed in the lower umbilicus of the ellipse and with a radius drawn to that center describe areas proportional to the times. And in the winter between the years 1683 and 1684 [this should be the winter between 1684 and 1685] this proposition with the demonstration was entered in the Register Book of the Royal Society.

---

[23] *Ibid.*, pp. 6-7. This manuscript is in Section I, Division XI, Number 41, of the Portsmouth Collection, among unarranged fragments of papers relating to the dispute with Leibniz about the invention of the "Method of Fluxions." After the quotation of this note in the *Portsmouth Catalogue* is the remark (p. xix): "Newton appears to have made one or two mistakes of date, and, probably for this reason, has drawn his pen through the entire passage." On the other side of the page of manuscript is a reference to Leibniz's death as having taken place. Leibniz died in 1716, so that Rouse Ball's above conjectured date would appear to be two years too early.

[24] "The question of gravitation," says Rouse Ball (*op. cit.*, p. 6), "was one of the problems of the time, hence it was natural that he should consider it. It is, however, probable that at this period his inquiries on the subject were of but a slight character, and would have passed almost unnoticed had they not proved the earliest steps to his later discoveries. His conclusions were not published, nor, as far as we know, are they contained in any manuscript now extant." The contributions of Kepler, Gilbert, Bouillaud, Borelli, and Hooke to the theory of gravitation before Newton are dealt with by Brewster, *op. cit.*, Vol. I, pp. 268-269, 282-288, and very much more fully by Rosenberger, *op. cit.*, pp. 135-157. Newton mentioned Borelli and Bullialdus (Bouillaud) in a letter to Halley (Rouse Ball, *op. cit.*, pp. 159-160).

"By this method [of fluxions] I invented the demonstration of Kepler's proposition in the year 1679, and almost all the rest of the more difficult propositions of the book of Principles in the years 1684, 1685, and part of the year 1686."

This account is not quite consistent with the more generally known and credited accounts of Whiston and Pemberton,[25] in which Newton is said to have thrown aside his calculation because of the discrepancy with observation—for he supposed a degree of latitude on the surface of the earth to contain sixty miles—and only to have taken them up again when the more exact measurements of Picard (1670) showed that the discrepancy was not very great after all.

Whiston and Pemberton's account was partly the basis of that given in Sir David Brewster's *Memoirs of Newton*:[26] "It was doubtless in the same remarkable year 1666, or perhaps in the autumn of 1665, that Newton's mind was first directed to the subject of gravity. He appears to have left Cambridge some time before the 8th of August 1665, when the college was dismissed on account of the plague, and it was therefore in the autumn of that year, and not in that of 1666, that the apple is said to have fallen from the tree at Woolsthorpe, and suggested to Newton the idea of gravity. When sitting alone in the garden and speculating on the power of gravity, it occurred to him that, as the same power by which the apple fell to the ground was not sensibly diminished at the greatest distance from the center of the earth to which we can reach, neither at the summits of the loftiest spires nor on the tops of the highest mountains, it might extend to the moon and retain her in her orbit in the same manner as it bends into a curve a stone or a cannon ball, when projected in a straight line from the surface of the earth. If the moon was thus kept in her orbit by gravitation to the earth, or, in other words, its attraction, it was equally probable, he thought, that the planets were kept in their orbits by gravitating towards the sun. Kepler had discovered the great law of the planetary motions, that the squares of their periodic times were as the cubes of their distances from the sun, and hence Newton drew the important conclusion that the force of gravity or attraction by which the planets were retained in their orbits varied as the square at the earth's surface. He was therefore led to compare it with the force exhibited in the actual motion of the moon, in a circular orbit; but, having assumed that the distance of the moon from the earth was equal to sixty of the earth's semidiameters, he found that the force by which the moon was drawn from its rectilineal path in a second of time was only 13.9 feet, whereas at the surface of the earth it was 16.1 in a second. This great discrepancy between his theory and what he then considered to be the fact, induced him to abandon the subject and pursue other studies with which he had been previously occupied."

---

[25] Rouse Ball, *op. cit.,* pp. 8-11. It is more convenient to refer to this reproduction of Whiston and Pemberton's accounts in the *Memoirs* (1749) and *View of Sir Isaac Newton's Philosophy* (1728) respectively.

---

[26] The passage referred to occurs in Vol. I, pp. 25-27 (2d ed., Vol. I, pp. 23-24); cf. pp. 290-291 (2d ed., Vol. I, pp. 253-254).

To the last sentence is attached a note on the story of the apple, to which we will shortly refer below.

Pemberton mentioned 1666 as the year in which the first thoughts that gave rise to the *Principia* occurred to Newton. This date was adopted by Rigaud, denied, as we have seen, by Brewster, and accepted, apparently on the grounds of the above manuscript account of Newton's, by Rouse Ball. Rigaud[27] remarks: "From Dr. Hodge's *Loimologia,* we learn that the plague began first in Westminster, at the end of 1664; that in London it was most violent in the hotter months of 1665, and had so far abated in the following winter that the inhabitants returned to their homes in December. The Royal Society did not indeed resume their regular meetings till March, 1666; but the disease had then subsided, and some writers have therefore referred Newton's speculation to 1665. Pemberton, however, is certainly right in assigning his retirement into Lincolnshire to the following year. In the sixth volume of the *Philosophical Transactions* there is a paper [already referred to] in which he says. . . .that he was forced from Cambridge by the plague [in 1666]. Further, Edleston[28] gives 1666 as the year in which the first idea of gravitation occurred to Newton.

I have devoted some space to what may appear the unimportant question as to whether Newton's first speculations on gravity took place in 1665 or 1666. The reason why the question is not unimportant arises from the fact that Newton had found, in essentials, his method of fluxions

[27] *Op. cit.,* note on p. 1.

[28] *Op. cit.,* p. xxi.

in 1665 and the beginning of 1666.[29] When we reflect that it was this method that formed the indispensable instrument for the discovery of the motion of a particle round a center of force, we see the importance of this. Newton was accustomed to the handling of problems involving infinitesimal or fluxional considerations; such considerations enabled him, since he stood at the new point of view in mechanics reached by Galileo, to calculate at once the relation afterwards published by Huygens between the force to the center of a circular orbit, the uniform velocity of a body describing this orbit and the radius of the orbit. Such considerations also showed—and this is a very important point—that there was now no great difficulty in extending calculations to orbs other than circular. Indeed, as Newton said in the postscript to his letter of June 20, 1686, to Halley, any mathematician could have concluded, from Huygens's discovery of the law of force in circular orbits, the manner of finding such laws even when the orbits were not circular.[30]

That Newton *did* take the inaccurate estimate is very probable.[31] After the feat of imagination involved in supposing that the power of gravity extends as high as the moon,[32] he seems to have started from Galileo's law of inertia, upon which he always seems—perhaps because of these questions—to have laid a great and logically unnecessary stress, thus showing that it may help our minds to have the proposition B emphasized even when we have already admitted the proposition A and we know that A implies B. As Whiston[33] says: "An inclination came into

---

[29] Brewster, *op. cit.*, Vol. I, p. 25.

[30] Rouse Ball, *op. cit.*, p. 160.

[31] Cf. *ibid.*, pp. 15-16.

[32] Cf. Mach, *op. cit.*, pp. 189-190, 513-514; Rouse Ball, *op. cit.*, pp. 9, 11. With regard to the origin of these speculations, the story of the apple, which Mach (*op. cit.*, p. 510) mentions with scorn, seems to have some foundation (cf. Rouse Ball, *op. cit.*, pp. 11-12). Cf. on this point Rosenberger, *op. cit.*, pp. 119-120; Rigaud, *op. cit.*, pp. 1-2; Brewster, *op. cit.*, Vol. I, note on p. 27. The story was first circulated by Voltaire.

[33] Rouse Ball, *op. cit.*, p. 8.

Sir Isaac's mind to try whether the same power did not keep the moon in her orbit, notwithstanding her projectile velocity and which he knew always tended to go along a straight line, the tangent of that orbit — which makes stones and all heavy bodies with us fall downward, and which we call gravity." Similarly Pemberton says:[34] "As he sat alone in a garden, he fell into a speculation on the power of gravity: that as this power is not found sensibly diminished at the remotest distance from the center of the earth to which we can rise, neither at the tops of the loftiest buildings, nor even on the summits of the highest mountains; it appeared to him reasonable to conclude that this power must extend much farther than was usually thought; why not as high as the moon, said he to himself, and, if so, her motion must be influenced by it; perhaps she is retained in her orbit thereby. However, though the power of gravity is not sensibly weakened in the little change of distance at which we can place ourselves from the center of the earth, yet it is very possible that, so high as the moon, this power may differ much in strength from what it is here. To make an estimate what might be the degree of this diminution, he considered with himself that if the moon be retained in her orbit by the force of gravity, no doubt the primary planets are carried round the sun by the like power."

As a first approximation, Newton supposed the moon to be a particle revolving in a circular orbit about the earth as center. In the investigation which followed, Newton seems to have proved independently Huygens's proposition about the centripetal acceleration in a central orbit, that[35]

$$\varphi = v^2/r.$$

It is not unlikely, as Rouse Ball[36] remarks, that Newton's proof of this relation was that referred to in his letter to Halley of July 14, 1686, when mentioning the form in which he proposed to get the scholium to the fourth proposition of the first book of the *Principia*: "In turning over some old papers, I met with another demonstration of that proposition, which I have added at the end of this scholium."[37] This proof is, as will be seen, based on infinitesimal principles:

---

[34] *Ibid.*, pp. 9-10.

[35] Mach, *op. cit.*, p. 160; *Ostwald's Klassiker*, No. 138, pp. 41-46, 74-76.

[36] *Op. cit.*, p. 13.

"In any circle suppose a polygon to be inscribed of any number of sides. And if a body, moved with a given velocity along the sides of the polygon, is reflected from the circle at the several angular points; the force with which at every reflection it strikes the circle will be as its velocity: and therefore the sum of the forces, in a given time, will be as the velocity and the number of reflections conjointly; that is (if the species of the polygon be given), as the length described in that given time, and increased or diminished in the ratio of the same length to the radius of the circle; that is, as the square of that length applied to the radius, and therefore if the polygon, by having its sides diminished *in infinitum,* coincides with the circle, as the square of the arc described in a given time applied to the radius. This is the centrifugal force with which the body impels the circle, and to which the contrary force, wherewith the circle continually repels the body towards the center, is equal."

I cannot see[38] any grounds for supposing that Newton, in these early investigations, drew the conclusion from Kepler's law of areas that this law implies a central acceleration and a uniform speed if the orbit were a circle. Indeed the law of areas was avowedly demonstrated by Newton at a much later date. It is not necessary to suppose that Newton had verified exactly the very natural conjecture that, if a body describes a circle uniformly, the deflective force is to the center of the circle. We must notice here that Kepler's laws were stated for the sun and the planets. That Newton considered the laws to apply also to the case of the earth and moon may perhaps be considered to be an indication that Newton had already extended the conception of gravitation to all matter, conformably to the third rule of his *regulae philosophandi*:

---

[37] *Ibid.,* p. 165.

[38] Cf. J. Cox, *Mechanics,* Cambridge, 1904 and 1909, pp. 90-91. On Newton's early investigations, see also pp. 85-86.

"....If it universally appears, by experiments and astronomical observations, that all bodies about the earth gravitate towards the earth, and gravitate in proportion to the quantity of matter which they severally contain; that the moon likewise, according to the quantity of its matter, gravitates towards the earth; that, on the other hand, our sea gravitates towards the moon; and all the planets mutually one towards another; and the comets in like manner towards the sun; we must, in consequence of this rule, universally allow that all bodies whatsoever are endowed with a principle of mutual gravitation. For the argument from the appearances concludes with more force for the universal gravitation of all bodies than for their impenetrability; of which, among those in the celestial regions, we have no experiments, nor any manner of observation."

Now, Kepler's third law (1619) is that the cubes of the major axes of the elliptical paths of the planets round the sun vary as the squares of the periodic times. Assuming that the earth bears much the same relation to the moon as the sun does to a planet, and that the orbit of the moon is circular, Kepler's third law states that

$$r^3/T^2 = k,$$

where r is the radius of the orbit, T the periodic time, v so that

$$v = 2\pi r/T,$$

and k is a constant. Substituting for v and then T in the above equation for φ, we get

$$\varphi = 4\pi^2/kr^2,$$

so that the force varies inversely as the square of the distance.

Newton[39] then calculated the space x through which the moon (considered as a particle) was drawn towards the earth in one second. He knew the space x' through which a particle near the surface of the earth was drawn by gravity in one second; it is about 16.1 feet. If his calculation showed that the ratio of x to x' was inversely as the ratio of the square of the distance of the moon from the earth's center, it would follow that the force which kept the moon in her (circular) orbit was the same as gravity. Newton made the numerical calculations, "though," as he says, "not accurately enough." Most likely they were made in the same way as that given in the fourth proposition of the third book of the *Principia*. This was as follows: The mean distance of the moon may be taken as $60a$, where a is the radius of the earth; the time of the moon is 39,343 minutes. Hence, in one minute the moon periodically describes an arc $2\pi . 60a/39343$, and, therefore, is drawn towards the earth through a distance equal to the versine of this arc. If a be taken as 4000 miles, this distance is about sixteen English feet, and it would follow that in one second the moon would be drawn through a distance x equal to $16/(60)^2$ feet; thus the moon and a particle near the earth would be drawn in one second through spaces very nearly inversely proportional to the squares of their distances from the earth's center; and the approximation is so close that we might reasonably infer that the force which keeps the moon in her orbit is the same as gravity. This verification requires that the value of a, the earth's radius, shall be known approximately. Now, Pemberton and Whiston agree that Newton assumed that a degree of latitude on the earth contains 60 miles (whereas, in fact, it contains about 69½ miles). This is equivalent to saying that Newton supposed that the radius of the earth was $10800/\pi$ miles, that is to say, rather more than 3,400 miles (whereas, in fact, it contains about 4000 miles). Hence his value of a was about one-eighth too small, and thus his calculations seemed to show that in one second the moon fell through only seven-eighths of the distance through which gravity alone would have pulled it. The calculation with his data gives $x =$ 13.9 feet.

---

[39] Cf. Rouse Ball, *op. cit.*, pp. 13-15; Cox, *op. cit.*, pp. 92-93.

This discrepancy does not appear, judging by Newton's own account given above, to have shaken his faith in the view that gravity, diminishing inversely as the square of the distance, acted on the moon, but led him to infer that some other force acted as well; and Whiston adds that Newton believed the other cause to be "Cartesius's vortices," while Pemberton states that Newton's "computation did not answer expectation; whence he concluded that some other cause must at least join with the power of gravity on the moon." But yet the view that Newton was quite disappointed by his calculation used to be generally held.

As regards the calculation of the distance fallen through by the moon in a second of time, Newton's object was to determine whether the moon, being deflected from the rectilinear direction of its motion, was drawn down towards the earth by a quantity which, according to the law which he assumed, would be consistent with the space through which heavy bodies in the same time have been found to fall at the earth's surface. The moon's distance being taken in semi-diameters of our globe, the absolute space through which it moved in its orbit during a given time was to be deduced from the magnitude of the earth. Now Newton took the common measure of sixty miles for a degree of latitude, which had been used by the old geographers and seamen. This was considerably too small, and consequently the distance by which the moon would under such circumstances have been drawn from the tangent of its orbit, came out less than the truth. Possibly the quantity which formed the foundation of his calculation had been the more impressed on his mind from its agreement with the result of the observations which Edward Wright had published in 1610. Wright was a Cambridge man—a fellow of Caius College—which may have made his work more familiar to the members of the University.[40] With regard to the measurement published by Norwood in 1636, Voltaire[41] remarked that the civil wars "had buried it in oblivion"; but in this respect Voltaire was certainly mistaken, as Rigaud[42] showed from the number of editions through which Norwood's work passed, and from contemporary evidence. The question as to how and when Newton became acquainted with Picard's measurement of 1671, and other questions, were very fully discussed by Rigaud.[43] However, the modern idea, first advocated by Adams, and based upon Newton's own manuscript note among the Portmouth papers, is that Newton did not lay great stress upon the close accuracy of his 1666 calculation, nor, consequently, upon Picard's new measurement. The whole question of the determination of forces and orbits seems to have appeared to him much more of merely a rough approximation than it actually was until, in 1685, he discovered his theorem on the attraction of spherical bodies. This will be considered more closely in the next section but one; and we will close the present section by giving a translation of Voltaire's account, in *Elémens de la Philosophie de Neuton,*[44] of Newton's early work on the theory of gravitation. This account is of great historical interest, as it was chiefly through Voltaire that Newtonianism became popular in France.

---

[40] Rigaud, *op. cit.*, p. 3. On the measurements of Snell and Norwood, see Rouse Ball, *op. cit.*, pp. 15-16. It may be mentioned that Newton possessed a copy of Robert Wright's (formerly of Jesus College, Cambridge) book *Viaticum Nautarun, or the Sailor's Vade Mecum* (see *Portsmouth Catalogue*, p. 56). I have hitherto been unable to find a copy of this work; the one in the Portsmouth Collection has been returned to Lord Portsmouth.

[41] See below.

[42] *Op. ct.*, pp. 4-5.

....[43] *Op. cit.*, pp. 6-12; cf. Brewster, *op. cit.*, Vol. I, pp. 290-291; Rouse Ball, *op. cit.*, pp. 22-23; *Principia*, Props. iv and xix, of Book III. Hooke, in his letter to Newton referred to below, of Nov. 24, 1679, alluded to Picard's geodetical measurements, and probably this led Newton to use Picard's estimate in the revision of his calculation. It should be mentioned that Snell's fairly accurate measure was mentioned in a book by *Varenius*, of which Newton brought out an edition in 1672 (Rouse Ball, *op. cit.*, p. 16).

"One day in the year 1666, Newton, having retired to the country and seeing the fruits of a tree fall, fell, according to what his niece, Mrs. Conduitt, has told me, into a deep meditation about the cause that thus attracts all bodies in a line which, if produced, would pass nearly through the center of the earth. What, he asked himself, is the force which cannot arise from these imaginary vortices that have been proved to be false?[45] It acts on all bodies in proportion to their masses and not to their surfaces; it would act on the fruit which has just fallen from this tree if the tree were raised 3000 fathoms (*toises*)[46] or even 10,000 fathoms. If that is so, this force must act towards the center of the earth in the place where the moon is: if this is so, this power —whatever it may be—may, consequently, be the same as that which makes the planets tend towards the sun and as that which makes the satellites of Jupiter gravitate to Jupiter. Now it is demonstrated by all the inductions deduced from Kepler's laws that all these secondary planets gravitate the more towards the center of their orbits as they are nearer and the less so as they are farther off, that is to say, reciprocally as the squares of their distances.

---

[44] First published in 1738. I quote from the new edition published at London in 1741, pp. 289-291.

[45] Voltaire, in fact, "proved" at the very beginning of the account of Newton's theory of gravitation that the Cartesian vortices and plenum are impossible.

[46] A *toise* is 6.39459 feet.

"A body placed in the position of the moon which revolves around the earth and a body placed near the earth must, then, both fall towards the earth exactly according to this law. Thus, to be assured if it is the same cause which retains the planets in their orbits and which makes heavy bodies fall to the earth, only measurements are necessary; we have only to examine what space is described by a heavy body falling to the earth in a given time and what space would be described, in a given time, by a body placed in the region of the moon. The moon itself is this body, and it may be considered as falling from its highest point of the meridian....

"It was thus that Newton reasoned; but he used, for the measurement of the earth, the faulty estimate of sailors, who counted sixty English miles for a degree of latitude, whereas it should have been seventy miles.

"There was, indeed, a more correct measurement of the earth. Norwood, an English mathematician, had measured a degree of the meridian fairly exactly in 1636 and had found it to be about seventy miles. But Newton did not know of this measurement made thirty years before. The civil wars which had afflicted England, and which are as harmful to the sciences as to the state, had buried in oblivion the only correct measurement of the earth that there was; and the rough estimate of sailors was kept. By this calculation the moon was too close to the earth, and the proportions sought by Newton were not given exactly. He did not think that he might add anything to fit nature to his ideas; he wished to fit his ideas to nature. He abandoned, therefore, the beautiful discovery which analogy with the other stars made so probable and to which so little was lacking for demonstration; and this rare quality of Newton's must give a great weight to his opinions.

"Finally, with the more exact measurements made in France, he found the demonstration of his theory.... Newton thus took up again the thread of his demonstration."

We must remember that this extremely naive and superficial account had—perhaps because of the enthusiasm with which it was written—an enormous influence on eighteenth century France. Indeed, its influence extended far beyond the boundaries of France; perhaps the conclusion—not wholly made legitimate, in this instance at least, by what appears really to have happened—as to Newton's sublime scientific restraint may not have been without influence on Cox's *Mechanics*.[47]

VI.

The first great mathematical advance of Newton's lay in the determination of the law of force for orbits which were not circular. The treatment of circular orbits is simple,[48] but in the more general case account has to be taken of a curvature which varies from point to point.

The conception of circle of curvature was introduced into geometry by Kepler in 1609,[49] and there seem to have been no other investigations on curvature published before 1665, when Newton applied his newly discovered fluxions to "the finding the radius of curvity of any curve." The remarks of Wallis,[50] which arose out of a controversy about the angle of contact of a circle with its tangent, are of later date than this, and were not published till 1685.

The normal to a given curve at a given point (P) will intersect with the normal at a neighboring point (Q) at some point (C′) on the concave side of the curve. As Q approaches P, C′ will—at least in the case of all the curves considered by Newton and by his successors until comparatively modern times—approach a limiting position C. The circle described with center C and radius CP will fit the curve more closely in the neighborhood of P than does any other circle. This circle is called "the circle of curvature at P." The "radius of curvature" (CP) is a measure of the curvature of the curve; the smaller the radius of curvature, the sharper is the turning of the curve. We find CP by finding the limit of C′P. Now

---

[47] Cambridge, 1904 and 1909.

[48] Cf. Mach, *Mechanics*, pp. 158-161.

[49] Moritz Cantor, *Vorlesungen über Geschichte der Mathematik*, Vol. II, 2d ed., Leipsic, 1900, p. 603.

[50] *Ibid.*, Vol. III, 2d ed., Leipsic, 1901, pp. 26-27; cf. Vol. II, p. 687.

$$\mathrm{C'P}/\mathrm{PQ} = \sin \mathrm{PQC'}/\sin \mathrm{PC'Q},$$

and PC′Q is equal to the angle between the tangents at P and Q. Further, as Q approaches P, the angle PQC′ approaches a right angle, and PC′Q ultimately becomes nearly equal to its sine and then vanishes. Hence, in the usual notation, we find for CP the value $ds/d\varphi$.

Notice that we have to imagine, when we are dealing with a body describing a non-circular orbit, that, in the infinitely small interval of time from t to $t + dt$, the body describes a small circular arc of length ds and radius r, where $r = ds/d\varphi$, $d\varphi$ being the small angle subtended by the arc ds at the center of the circle. In general, r will vary from point to point; if the orbit is such that $ds/d\varphi$ is the same as s/φ, so that r is the same however large the arc s may be, the orbit is circular. We see, then, that we have only to apply simple infinitesimal considerations to pass from circular orbits, considered by Huygens and Newton independently, to the general case.

It may be added that the "curvature" at a point is usually defined to be $d\varphi/ds$ or $1/r$; and that the radius of curvature may be expressed in terms of the rectangular coordinates by means of the well-known formulae $ds^2 = dx^2 + dy^2$, and $\tan\varphi = dy/dx$, so that

$$d\varphi = [(d^2y/dx^2)dx]/[1 + (dy/dx)^2].$$

Now, let v be the tangential velocity of the body at P and $v + dv$ that at Q, and notice that $v = ds/dt$. The resolved part of the velocity at Q in a direction parallel to the normal at P is $(v + dv)\sin(d\varphi)$ or, neglecting the infinitesimal of the second order, $v.d\varphi$. The normal acceleration, then, at P is $(v.d\varphi)/dt$, or

$$v(d\varphi/ds)(ds/dt) = v^2/r,$$

where r is the radius of curvature at P. This is a generalization of the theorem of Huygens and Newton.

Another way of obtaining the same result is to call f the central acceleration, and to notice that the tangential velocity at Q is the result of compounding the tangential velocity v at P and the velocity $f.dt$ perpendicular to it. Now

$$(f.dt)/v = \tan(d\varphi) = d\varphi,$$

and, remembering that the angle PC′Q is equal to $d\varphi$,

$$d\varphi = (v.dt)/r;$$

so

$$f = v^2/r.$$

VII.

We now meet the question, referred to in the preceding section, which has done much towards the changing of our point of view towards the long delay and perhaps the lack of interest of Newton in the pursuit of the theory of gravitation.

In a letter to Halley of June 20, 1686, Newton wrote:[51] "....I never extended the duplicate proportion lower than to the superficies of the earth and, before a certain demonstration I found the last year, have suspected that it did not reach accurately enough down so low; and therefore in the doctrine of projectiles I never used it nor considered the motions of the heavens;...."

---

[51] Rouse Ball, *op. cit.*, p. 157.

The theorem in question contains the discovery that a spherical mass attracts an external particle as if the whole mass were concentrated at the center. We must remember that every particle of the mass is supposed to attract, so that this result is as simple as it is beautiful. Newton's papers and correspondence of 1679 and 1685 show, according to Rouse Ball,[52] that at this time he did not know how to determine the attraction of a solid body, like the sun or earth, on an external particle; he did not think that the earth's attraction was directed exactly to its center; and it is probable that, previously to 1685, he considered that the attraction of a planet on its moon (or of the sun on a planet) could be regarded as directed to a fixed point, and varying as the inverse square of the distance from it, only when the two bodies were at a distance so great that their dimensions could be neglected compared with the distance between them.

The extension of the idea of gravitation to the smallest particles of bodies is not only natural, but even seems to be, speaking psychologically, inevitable. Further, Newton's calculations on the moon's orbit were only very rough, and he could hardly have been deterred from following up the new idea by the merely approximate character of the result. These two considerations, combined with the probable cause mentioned above for the cessation of Newton's investigations for thirteen years, seem to have induced the celebrated English astronomer John Couch Adams[53] to believe that Pemberton and Whiston were mistaken as to the insufficiency of the verification. Newton knew that the orbit was not actually circular, and that his numerical data were only approximate; hence he could have expected only a rough verification of the hypothesis, and, as he asserted that he found his results agree or "answer pretty nearly," Adams considered that these calculations were sufficient to convince Newton that it was gravity alone that retained the moon in its orbit, and further, he strongly suspected that Newton already believed that gravity was due to the fact that every particle of matter attracts every other particle, and that this attraction varies as the product of the masses and inversely as the square of the distance between them. We will return to this point further on.

---

[52] *Ibid.*, p. 11.

[53] Rouse Ball, *op. cit.*, pp. 16-17.

VIII.

It is important to bear in mind the fact that Newton could not have made use of the researches of Huygens on centrifugal force. Newton's first speculations and calculations on gravitation began in 1666, while Huygens's theorems on centrifugal force were given[54] in 1673, in the fifth part of the *Horologium oscillatorium*— but without proof—and a fairly complete "Tractatus de vi centrifuga" was only published in Huygens's *Opuscula Postuma* of 1703.[55]

A reader of Mach's *Mechanics*[56] might conclude that Newton followed in the footsteps of Huygens; but in point of fact it was Galileo's work alone which was the starting point for Newton's early reflections on gravitation and the moon's orbit.

[54] Some theorems were communicated by Huygens to the London Royal Society in 1669 in the form of an anagram.

[55] A convenient annotated German translation of this tract is given in No. 138 of *Ostwald's Klassiker.*

[56] P. 188. Mach here says: "He who clearly understands the doctrine of Galileo and Huygens, must see that a curvilinear motion implies deflective acceleration. Hence, to explain the phenomena of planetary motion, an acceleration must be supposed constantly directly towards the concave side of the planetary orbits." Of course this is not a statement that Newton made use of Huygens's discoveries, but it might be understood as implying that he did so. Dühring (*op. cit.,* p. 173) wrongly stated that Newton's theory partly grew out of Huygens's theory of motions in a circle under a central force. The fact that the beginnings of Newton's investigations of centripetal forces

"It is not merely," says Brewster,[57] "from his astronomical discoveries, brilliant as they are, that Galileo claims a high place in the history of Newton's discoveries. His profound researches on mechanical science—his determination of the law of acceleration in falling bodies—and his researches respecting the existence and cohesion of solid bodies, the motion of projectiles, and the center of gravity of solids, have ranked him among the most distinguished of our mechanical philosophers. The great step, however, which he made in mechanics was the discovery of the general laws of motion uniformly accelerated, which may be regarded as the basis of the theory of universal gravitation." Indeed, it seems that nobody can seriously maintain that Galileo's mechanical researches did not exert a far more important influence on Newton's work than did his astronomical discoveries. Undoubtedly, descriptions of the sun, moon, and planets are more striking to an ordinary person than a description of the law followed by falling bodies near the surface of the earth; but there can be no comparison between the essential importance to science of these descriptions. And we shall see that the correct mechanical principles to be followed in the formulation of the problem of finding the path of a particle which moves under the influence of a central attraction was familiar, for example, to Wren, Hooke, and Halley. These men were stopped by mathematical difficulties; the ideas of the fluxional or infinitesimal calculus were necessary for the progress of mechanics. It was fortunate that Newton had become familiar with the help that the conception of motion may offer in the solu-

---

were prior to the publication of Huygens's work was emphasized by J. Cox in his *Mechanics* (p. 90), a text-book written on the historical lines laid down so splendidly by Mach, and thus combining teaching, history, and criticism in a welcome manner. Cox (*op. cit.*, pp. 88-89) also laid stress on Galileo's law of inertia, which was and is so important psychologically to any one attacking problems of celestial mechanics.

[57] *Op. cit.*, Vol. I, pp. 273-274.

tion of certain geometrical problems and had invented the method of fluxions at a very early date.

IX.

With regard to the progress that Newton made between 1666 and 1679 in his ideas on gravitation, we have clear indications from Newton's own letters that he was familiar with the fact that gravitation varied inversely as the square of the distance by 1673 at the latest. When Huygens published his *Horologium oscillatorium* in 1673, he presented a copy to Newton, and Newton, in his letter of thanks, said:[58] "I am glad that we are to expect another discourse on the *Vis centrifuga,* which speculation may prove of good use in natural philosophy and astronomy, as well as mechanics. Thus, for instance, if the reason why the same side of the moon is ever towards the earth be the greater *conatus* of the other side to recede from it, it will follow (upon supposition of the earth's motion about the sun) that the greatest distance of the sun from the earth is to the greatest distance of the moon from the earth not more than as 10,000 to 56, and therefore the parallax of the sun is not less than 56/10,000 of the parallax of the moon; because, were the sun's distance less in proportion to that of the moon, she would have a greater *conatus* from the sun than from the earth. I thought also some time that the moon's liberation might depend upon her *conatus* from the sun and earth compared together till I apprehended a better cause."

The rest of this letter, which, for the most part, is on the subject of colors, was printed in the *Philosophical Transactions* of July 21, 1673. From the above words, "it is evident," continues Newton in his letter to Halley,

---

[58] The copy of this letter to Huygens was given in a letter of Newton to Halley of July 27, 1686 (Rouse Ball, *op. cit.*, p. 166). Before Newton found the copy of this letter, he gave from memory, in his letter to Halley of June 20, 1686, a slightly different account which is referred to below.

"that I was at the time versed in the theory of the force arising from circular motion, and had an eye upon the forces of the planets, knowing how to compare them by the proportions of their periodical revolutions and distances from the center they move about: an instance of which you have here in the comparison of the forces of the moon arising from her menstrual motion about the earth and annual about the sun."

The "better cause" mentioned in the above letter to Huygens was communicated in 1675 to Nicolas Mercator who published it in the following year in his *Institutiones astronomicae*. Galileo had discovered and explained the diurnal libration, arising from the spectator not viewing the moon from the center of the earth, but it was reserved for Newton to explain the libration in longitude which Hevelius, its discoverer, had ascribed to the displacement of the center of the moon's orbit from the center of motion. Newton showed that it was occasioned by the inequalities of the moon's motion in an elliptic orbit round the earth, combined with the uniformity of her motion round her axis. In the same letter to Mercator, he showed that the libration in latitude arose from her axis of rotation being inclined 88° 17′ to the ecliptic.

Before Newton had found the copy of his letter of 1673 to Huygens, he wrote to Halley on June 20, 1686, that he "gave those rules in the end [of Huygens's book] a particular commendation for their usefulness in philosophy, and added out of my aforesaid paper [an early manuscript, probably of 1666] an instance of their usefulness, in comparing the forces of the moon from the earth, and earth from the sun, in determining a problem about the moon's phase, and putting a limit to the sun's parallax; which shows that I had then my eye upon comparing the forces of the planets arising from their circular motion, and understood it."[59] In a postscript to this letter, Newton added: "It is more than I can affirm that the duplicate proportion was not expressed in that letter."[60]

However, in the letter of July 27, 1686, Newton acknowledged: "Now, though I do not find the duplicate proportion expressed in this letter (as I hoped I might), yet, if you compare this passage of it here transcribed with that hypothesis of mine, registered by Mr. Oldenburg in your books, you will see that I then understood it. For I there suppose that the descending spirit acts upon bodies here on the superficies of the earth with force proportional to the superficies of their parts; which cannot be, unless the diminution of its velocity in acting upon the first parts of any body it meets with be recompensed by the increase of its density arising from that retardation. Whether this be true is not material. It suffices that it was the hypothesis. Now, if this spirit descend from above with uniform velocity, its density, and consequently its force, will be reciprocally proportional to the square of its distance from the center. But if it descend with accelerated motion, its density will everywhere diminish as much as its velocity increases; and so its force (according to the hypothesis) will be the same as before, that is still reciprocally as the square of its distance from the center."[61]

The hypothesis referred to in this letter was contained in the discourse presented in manuscript dated the 7th of December, 1675, to the Royal Society, with the title: "A Theory of Light and Colours, containing partly an Hypothesis to explain the properties of light discoursed of by him in his former papers, partly the principal phenomena of the various colours exhibited by thin plates or bubbles, esteemed to be of a more difficult consideration, yet to depend also on the said properties of light."[62]

---

[59] Rouse Ball, *op. cit*, pp. 157-158. [60] *Ibid.*, p. 160. [61] *Ibid.*, pp. 166-167.

[62] This paper was read on Dec. 9, 1675 (Birch, *op. cit.*, Vol. III, pp. 247, 260), and registered in the January or February following.

It is curious to notice that not long before this date Collins informed James Gregory that Newton and Barrow had "begun to think mathematical speculations at least dry, if not somewhat barren."[63] The hypothesis,[64] together with the controversy with Hooke to which it gave rise, belongs more particularly to the subject of physical optics; but the subject of gravity was occasionally referred to. An ether was assumed, and its heterogeneity seemed to be required by "the electric and magnetic effluvia, and the gravitating principle.[65] Again, there occurred the paragraph:[66] "And as the earth, so perhaps may the sun imbibe this spirit copiously to conserve his shining and keep the planets from receding further from him; and they that will may also suppose that this spirit affords or carries thither the solary fuel and material principle of light. And that the vast ethereal spaces between us and the stars are for a sufficient repository for this food of the sun and planets. But this of the constitution of ethereal natures by the by."

Newton quoted this paragraph in the postscript of the letter written to Halley on June 20, 1686, with the remark: "In these and the foregoing words you have the common cause of gravity towards the earth, sun, and all the planets, and that by this cause the planets are kept in their orbs about the sun. And this is all the philosophy Mr. Hooke pretends I had from his letters some years after, the duplicate proportion only excepted. The preceding words contain the cause of the phenomena of gravity, as we find it on the surface of the earth, without any regard to the various distances from the center. For at first I designed to write of nothing more. Afterwards, as my manuscript shows, I interlined the words above cited relating to the heavens; and, in so short and transitory an interlined hint of things, the expression of the proportion may well be excused. But if you consider the nature of the hypothesis, you will find that gravity decreases upwards, and can be no other from the superficies of the planet than reciprocally duplicate of the distance from the center, but downwards that proportion does not hold. This was but an hypothesis, and so to be looked upon only as one of my guesses, which I did not rely on; but it sufficiently explains to you why, in considering the descent of a body down to the center, I used not the duplicate proportion. In the small ascent and descent of projectiles above the earth, the variation of gravity is so inconsiderable that mathematicians neglect it. Hence the vulgar hypothesis with them is uniform gravity. And why might not I, as a mathematician, use it frequently, without thinking on the philosophy of the heavens, or believing it to be philosophically true?"[67]

---

[63] Brewster, *op. cit.,* p. 127.

[64] The "Hypothesis" was reprinted by Brewster on pp. 390-409; and an extract from Birch's *History* (Vol. III, pp. 249-251) in Rigaud (*op. cit.,* Appendix No. xx, pp. 68-70).

[65] Brewster, *op. cit.,* p. 392.

[66] *Ibid.,* p. 394.

In the body of this letter, Newton said:[68] "Between ten and eleven years ago, there was an hypothesis of mine registered in your books, wherein I hinted a cause of gravity from the superficies of the planets (though for brevity's sake not there expressed) can be no other than reciprocally duplicate of the distance from the center. And I hope I shall not be urged to declare, in print, that I understood not the obvious mathematical conditions of my own hypothesis."

In a letter to Robert Boyle, which was written on February 28, 1679,[69] Newton said: "I shall set down one conjecture more which came into my mind now as I was writing this letter: it is about the cause of gravity. For this end I will suppose ether to consist of parts differing from one another in *subtlety,* by indefinite degrees; that in the pores of bodies there is less of the grosser ether in proportion to the finer than in the regions of the air; and that yet the grosser ether in the air affects the upper regions of the earth and the finer ether in the earth the lower regions of the air, in such a manner that from the top of the air to the surface of the earth, and again from the surface of the earth to the center thereof, the ether is insensibly finer and finer. Imagine, now, any body suspended in the air or lying on the earth; the ether, being by the hypothesis grosser in the pores which are in the upper parts of the body than in those which are in its lower parts, and that grosser ether being less apt to be lodged in those pores than the finer ether below, it will endeavor to get out and give way to the finer ether below, which cannot be, without the bodies descending to make room above for it to go out into."

---

[67] Rouse Ball, *op. cit.*, pp. 161-162.

[68] *Ibid.*, p. 158.

[69] This letter is reproduced by Brewster, *op. cit.*, pp. 409-419. The passage given is on pp. 418-419.

In this letter to Boyle, Newton deduced, from the rarification of the ether between two bodies which approach one another, firstly, an endeavor of these bodies to recede from one another, and, secondly, their adherence to one another.[70]

On this passage Rosenberger[71] remarks: "This deduction of molecular attraction and repulsion shows clearly how far Newton then was from his system of primitive or elementary forces, which only differed by the law of the actions, of all matter."

It is to be noticed that Newton himself, although he rejected the undulatory theory of light because he did not think that this theory could explain the rectilinear propagation of light, considered the whole of space to be filled with an elastic medium which propagates vibrations in a manner analogous to that in which the air propagates vibrations of sound. The ether penetrates into the pores of all material bodies, whose cohesion it brings about, it transmits gravitational action, and its irregular turbulence constitutes heat. Light, although it is not itself constituted by vibrations of this medium, but is composed of currents of corpuscles emitted by luminous bodies, freely traverses the ether. The ether has a density so small that it does not offer any sensible resistance to the heavenly bodies which move in it.[72]

---

[70] Brewster, *op. cit.*, pp. 412-413.

[71] *Op. cit.*, p. 125.

Here may be mentioned two facts which, indirectly or directly, concern theories of ether with Newton and his predecessors.

Bullialdus derived the conclusion that the force of gravity varies inversely as the square of the distance by analogy with the diminution that occurs in the intensity of light; the opinions of Robison and Leibniz on the validity of this argument were given by Rigaud.[73] The hypotheses framed by Bullialdus, Huygens, Leibniz, John Bernoulli, and Newton, to devise some mechanical cause for the effects of gravitation were also noticed by Rigaud.[74]

X.

About 1677, Donne and Newton visited Wren at his lodgings, and Wren "discoursed of this problem of determining the planetary motions upon philosophical principles." This was stated in a letter from Newton to Halley of May 27, 1686,[75] and Newton added: "You are acquainted with Sir Christopher. Pray know where and whence he first learnt the decrease of the force in a duplicate ratio of the distance from the center."

---

[72] Cf. E. T. Whittaker, *A History of the Theories of Aether and Electricity from the Age of Descartes to the Close of the Nineteenth Century*, London and Dublin, 1910. Cf. also p. 534 of Mach's *Mechanics*.

[73] *Op. cit.*, p. 56.

[74] *Op. cit.*, pp. 59-63, and Appendix, No. xviii, pp. 65-66, No. xx, pp. 68-70, and No. xvii, pp. 62-64 (letter to Boyle).

[75] Rouse Ball, *op. cit.*, p. 156.

Halley replied to this request on June 29, 1686:[76] "According to your desire in your former, I waited upon Sir Christopher Wren to inquire of him if he had the first notion of the reciprocal duplicate proportion from Mr. Hooke. His answer was that he himself, very many years since, had had his thoughts upon the making out the planets' motions by a composition of a descent towards the sun and an impressed motion; but at length he gave over, not finding the means of doing it. Since which time Mr. Hooke had frequently told him that he had done it, and attempted to make it out to him, but that he never satisfied him that his demonstrations were cogent. And this I know to be true, that in January 1683-4, I, having from the consideration of the sesquialter proportion of Kepler concluded that the centripetal force decreased in the proportion of the squares of the distances reciprocally, came one Wednesday to town, where I met with Sir Christopher Wren and Mr. Hooke, and, falling in discourse about it, Mr. Hooke affirmed that upon that principle all the laws of the celestial motions were to be demonstrated, and that he himself had done it. I declared the ill success of my attempts; and Sir Christopher, to encourage the inquiry, said that he would give Mr. Hooke or me two months' time to bring him a convincing demonstration thereof, and besides the honor, he of us that did it should have from him a present of a book of forty shillings. Mr. Hooke then said that he had it, but he would conceal it for some time that others, trying and failing, might know how to value it when he should make it public; however I remember Sir Christopher was little satisfied that he could do it, and, though Mr. Hooke then promised to show it him, I do not yet find that in that particular he has been as good as his word."

PHILIP E. B. JOURDAIN.

CAMBRIDGE, ENGLAND.

---

[76] *Ibid.*, pp. 162-163.